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NOTES FROM THE MILLPORT MARINE BIOLOGICAL STATION.

By RICHARD ELMHIRST, Superintendent.

ON THE YAWNING OF FISHES.

WHILE watching the fish living in our Aquarium, I have repeatedly observed them yawning, or at any rate perform an action like yawning, consisting of a wide opening of the mouth, slow expansion of the buccal cavity, erection of the gill-arches, followed by a rapid expulsion of the indrawn water, most of which is emitted from the mouth, although some certainly goes through the gill-slits. This is often accompanied by a distinct heaving of the pectoral region and erection of the pectoral fins, and is quite different from the rapid movement of the operculum and jaws which is used to remove a foreign object, such as a bit of seaweed, from the gills. At first one would think that yawning is only possible for an air-breathing vertebrate with lungs. However, from numerous observations, I am led to think that this action of fishes is a real yawn, and serves the true physiological purpose of a yawn, *i. e.* flushing the brain with blood during periods of sluggishness. The conditions conducive to yawning are a slight increase in the temperature of the water and, I suppose, the accompanying diminution of oxygen. For instance, on Saturday morning we flush a lot of water through our tanks, and at midday leave the storage tank full. Now, we usually use a tankful in twenty-four hours, but to avoid pumping on Sunday we make the tankful last from

Saturday midday to Monday morning. Now, as this tank full of water is exposed to the sun on Saturday afternoon and all Sunday, its temperature rises in summer to 6° or 8° F. above that of the sea, and it is then that I have most often noticed the fishes yawning. I find also that when any individual fish is yawning frequently that the rate of its respiratory action is slower than usual. By respiratory action I mean the intaking of water at the mouth, and its emission through the gill-slits. For instance, I find the average rate for a certain Plaice is thirty-two per minute, and twice when yawning frequently it was as low as twenty per minute. A Dab has an average of about forty-two, and when yawning it was thirty-one. Sometimes, however, a fish yawns when the number of respiratory actions per minute is above the average. Cod seem to average about thirty-six per minute, whether yawning or not; I think they yawn much oftener than any others.

Whether the psychological infectiousness of yawning holds good among fishes I cannot say. I have certainly seen several yawn frequently, oftener than I have seen one give a single yawn. But this may be explained by the conditions conducive to yawning affecting several of the fish. This action is so suggestive that on seeing it I often start yawning myself. I find that a dog yawning before one induces yawning, although I cannot say that I have ever induced my dog or the fishes to yawn by yawning at them. I suppose human beings are probably more susceptible to such influences than the lower vertebrates. When several of a number of fish are yawning frequently, if one does anything to attract their attention all yawning ceases, *e. g.* if one performs actions as though going to feed them, they become excited, as when feeding is going to take place, swimming rapidly, following one, and making snapping movements as though seizing food. (From this it is quite clear that fish display an intelligent interest in what goes on outside the tank.) This sudden activity is accompanied by an increased rate of the respiratory action, which I suppose sets up a stronger circulation of the blood, and in this way relieves the sluggishness and its accompanied yawning.

The following list of the occurrence of yawning among vertebrates may be of interest. In the tanks at Millport the

Cobbler (*Cottus scorpius*), Saithe (*Gadus virens*), Lythe (*G. pollachius*), Cod (*G. callarius*), Ballan Wrasse (*Labrus bergylta*), Goldsinny (*Ctenolabrus rupestris*), Plaice (*Pleuronectes platessa*), Dab (*P. limanda*), Conger Eel (*C. niger*), and Skate (*Raia clavata*) have all been seen yawning by Mr. A. Gray (late Curator), Mr. J. Peden (Laboratory Attendant), various visitors, or myself.

Mr. G. A. Dunlop has observed the same action in Carp (*Cyprinus carpio*), Roach (*Leuciscus rutilus*), and Rudd (*L. erythrophthalmus*) in aquaria at Warrington Museum. Professor J. Graham Kerr has noticed a similar action in *Lepidosiren*.

Among reptiles and amphibians I have no written records, but have a vivid impression of seeing a Lizard and Newts yawn; I think the Common Frog and Grass Snake also. I seem, too, to have seen a photograph of a yawning Crocodile.

Mr. Gray keeps a tame Kittiwake which often yawns in the morning, just as fowls when first let out often yawn, stretch, and flap their wings. Mr. C. Kirk tells me Owls yawn, and he has published an excellent photograph of a young Carrion-Crow (*Corvus corone*) yawning in 'Gowans's Nature Books,' No. 19, p. 23. While working here in August, Mr. Dunlop saw a Lesser Black-backed Gull yawn.

Dogs, Cats, and their relations in zoological gardens often yawn. I am told that Horses, Cattle, and Goats yawn, although I personally never remember seeing them do so; I cannot find any records of a rodent yawning.

LITTORAL FEEDING HABITS OF SOME LAND-BIRDS.

During last autumn I frequently noticed Starlings feeding between tide-marks, and later, during severe wintry weather, Robins, Blackbirds, and Thrushes as well. The Starlings were generally in small flocks of six or more. During the spells of dry weather this year (*e. g.* June 1st to 18th, rainfall .06 in.) numbers of Starling families fed regularly between tide-marks. On June 10th and subsequent days I watched the young ones being fed. The adults are expert shore-collectors, and catch *Gammarus*, *Idotea*, *Ligia*, *Littorina*, &c., for the young birds, who sit about the stones, and in turn follow the old bird to be fed; the favoured individual displaying considerable excitement. Young Jackdaws also frequent the shore, but they go about tips and

scavenge rather than collect animals. Old Blackbirds fed on the shore, and also took food away with them.

It will be interesting to find out if land-birds ever become infected with helminth or other parasites through feeding on the shore. Miss M. V. Lebour has lately examined four Starlings for trematodes with no result. I find the *Acanthocephalan*, *Echinorhynchus cylindraceus*, Schrank, is common in the Starling, and also occurs in the Blackbird. I have observed stomach contents as follows :—

Starling	I. Nov. 1909.	<i>Gammarus</i> , also beetle remains, and tip refuse.
„	II. „ „	<i>Lacuna divaricata</i> , <i>Gammarus</i> .
„	III. June, 1910.	Four <i>Gammarus marinus</i> .
„	IV. „ „	Six <i>Littorina obtusata</i> , two <i>L. rudis</i> , <i>G. marinus</i> , <i>G. locusta</i> ; also a Thysanuran and a Lepidopteran larva.
„	V. „ „	Over fifty <i>Gammarus</i> , five beetles.
„	VI. Aug. 1910.	Ten <i>L. rudis</i> , over fifty <i>Gammarus</i> , insect remains.
„	VII. „ „	One <i>Rissoa cingillus</i> , twenty <i>Gammarus</i> , and beetle remains.
„	VIII. „ „	Six <i>Gammarus</i> , pips and bits of apple-skin.
Blackbird, Nov. 1909.		<i>Lacuna divaricata</i> .

This list shows that they feed much as the shore-birds do. Last November a Purple Sandpiper contained sponge spicules and some *Gammarus*, and a Redshank contained *Idotea*, *L. obtusata*, and *L. rudis*. Among the weeds near high-water mark, where the Starlings feed so abundantly on *Gammarus*, the Turbellarian (*Gunda ulvæ*) is very common, yet they do not appear to touch them.



VARIATIONS IN THE DENTITION OF *ERINACEUS EUROPAEUS*.

By EDWIN HOLLIS, F.Z.S.

My attention having been attracted to some curious variations in the teeth of some Hedgehogs taken in the neighbourhood of Exeter, I consulted several books, and found that they all give as a characteristic the fact that *E. europæus* differs from the other members of the *Erinaceidæ* in having the third upper incisors and canines single rooted, whereas in other species these have two distinct roots.

The following extracts (from 'Mammals Living and Extinct,' Flower and Lydekker) bring out the above distinctions very clearly. In writing of the *Erinaceidæ* they say :—

"The canine is very similar to the third incisor, and except in *E. europæus* each of these teeth is inserted by two distinct roots."

"*E. europæus* is the most aberrant species, differing from all the rest in the peculiarly shaped and single rooted third upper incisor and canine."

"The Indian form, *E. collaris*, may be considered characteristic of all the other species, the only important differences being found in the variable size and position of the second upper pre-molar, which is very small, external and deciduous in *E. micropus* and *pictus*."

To emphasize the above points, they figure *E. collaris* (p. 620, fig. 280) and *E. europæus* (p. 620, fig. 285), both after "Dobson, Proc. Zool. Soc. Lond. 1881."

The specimens I had under observation appeared to show characters placing them midway between *E. europæus* and the other species as above described. Thinking that this might indicate that these were of a distinct local race peculiar to Devonshire, I obtained further specimens from Sussex, Northampton, and Bucks, but found on examination that these presented the same peculiarities.

I give below a table showing the peculiarities of eleven skulls which I have now before me :—

	3rd Incisors.	Canines.	2nd Premolars.	
			Right.	Left.
1. Exeter	Single rooted	Double rooted	Rudimentary	Rudimentary
2. do.	do.	do.	Absent	Absent
3. do.	do.	do.	Normal	Normal
4. Aylesbury	do.	do.	Absent	do.
5. do.	do.	{ Single rooted, but showing signs of fu- sion of the two roots }	Normal	do.
6. Wellingborough	do.	do.	do.	do.
7. Horsham.....	do.	do.	do.	do.
8. do.	do.	do.	do.	do.
9. do.	do.	Double rooted	do.	do.
10. do.	do.	do.	Absent	Absent
11. Wellingborough	do.	Single rooted	Normal	Normal

It will be seen from the above table that only one specimen (No. 11) conforms to the type as described. I have since, by the courtesy of Mr. Oldfield Thomas, been allowed to inspect a considerable number of skulls in the collection of the British Museum (Natural History). I then found that a large proportion, probably half, of the skulls had double rooted canines, one having a single root on one side and double on the other, also that several show variations in the size of the second upper premolar. This tooth was in some cases extremely small and crowded for space, but, so far as I observed, in no case entirely absent, as in my specimens, Nos. 2 and 10.

Bearing in mind the third paragraph quoted above, I think this variation of the second premolar, which can be traced through all stages, from normal size to absence, is very interesting, and probably points to *E. europæus* being in a state of transition to a condition similar to that mentioned as occurring in *E. micropus* and *pictus*.

From the above observations it is evident that the teeth of *E. europæus* are extremely variable, and that the fact of the incisors being single or double rooted will no longer hold good as distinguishing this species from all others.

It will be interesting to see whether the study of a larger series of skulls will show any specimens with double rooted third incisors; if it does not do so, this may perhaps still be taken as a distinguishing character, provided that the study of a large series of skulls belonging to other species shows that in them the third incisor is invariably double rooted.

HUMBLE-BEES AND FOXGLOVES.

BY EDMUND SELOUS.

Not being an expert in the Hymenoptera, and having been quite possibly (or even probably) mistaken in the correct scientific names of some of the bees whose actions are here recorded, I should like at the outset to point out that the interest of the observations contained in the following notes lies, not in knowing what bees do certain things, but what things certain bees do. It is, of course, a very good thing to be sure of the species that one is observing. One should always be so, if one can. Sometimes, however, one cannot, but that does not take away all value from what one has seen, except in some special cases where the identity of the species is all-important. Otherwise, an anonymous fact in natural history is not less interesting, on that account, than, in the domain of literature, an anonymous novel, for instance, may be.

Whilst staying at Frendenstadt, in the Black Forest, during the summer of 1907, I watched Humble-Bees visiting foxgloves, over a certain limited area where these grew thickly, to the exclusion of other flowers. The two species most frequently seen here were *Bombus hypnorum*, and another large Humble-Bee with a dusky, yellowish patch on the thorax, and a somewhat long and curved abdomen, the specific identity of which I have not been able to ascertain. The latter was much the commoner of the two, and I have nothing further to record of it than that it invariably, according to my observation, rifles the foxglove in the ordinary manner, by which I mean that, in order to do so, it first enters the mouth of the elongated sack or "glove" formed by the conjoined petals. *B. hypnorum* also usually enters the flowers, but individuals are to be seen which go, apparently by preference, to the exposed green calyces from which the blossom has dropped.

B. terrestris is less common there than with us. The first individual I particularly noticed was visiting the exposed calyces,

and before I had made any further study of its habits, my attention was drawn to a quite small Black Bee (*B. mastrucatus*, according to the nomenclature of the zoological department of the museum at Stuttgart), which interested me by invariably going to the neck or tube of the corolla, on the outside, and piercing it (*as it seemed*) with its proboscis. Whether it really did so, however, or only took advantage of a hole that had already been made there, it was difficult to be certain of. In many cases the latter plan was certainly adopted, but then it naturally would have been, if the flower had been previously visited and pierced by another bee.

The movements of this small bee were very quick, nor did it stay long at any one spike of the foxglove, but soon darted away from it to another, usually at some distance off. Moreover, during the time that I was able to watch it at any one, it was extremely difficult, if not impossible, to see the first actual entry of the proboscis into the tube of the flower, or to make sure, between the time of this happening and that of the bee settling, that the tube had not been previously perforated. On one point I soon satisfied myself, *viz.* that the bee did not first bite a hole, and then insert its proboscis into it, which, armed as it is with mandibles, and accustomed to use them, it might have been expected to do.

Continuing my observations, I soon found that it was not only the small Black Bee I have mentioned (*B. mastrucatus*) that obtains the nectar or juice of the foxglove in this way, but also *B. terrestris*, which I had before seen visiting the naked ovaries, to the neglect of those still enclosed by the blossom. I watched various individuals thus acting during the greater part of an afternoon, and noticed that it was not every flower on which they settled that they were able to perforate. In many instances they would feel about with their proboscis, as though seeking an aperture, and, failing to find it, fly to another one. When I picked these flowers and examined them, I found that they were intact, but the same bee, upon finding a hole in another, would at once pass her proboscis through it. As, therefore, the bees do not use their mandibles, and must know that they cannot of themselves pierce the tube of the corolla in any other way, it seems evident that they consciously search for

such of these as have been pierced, passing the others by. They never, in any case, enter the corolla in the orthodox manner, after finding that it is not perforated—that is to say, I have never seen one do so. The same remarks apply to *B. masticatus*.

Besides the above species of Humble-Bee there is another, not very common, which I have not been able to identify. It is brown, like *B. hypnorum*, but not more than half the size even of the smaller forms of this, between which and the larger there is a considerable difference. It is also less furry than *B. hypnorum*, or than Humble-Bees generally, such fur as it has being mostly on the thorax. This small bee seems never to enter the foxglove, but settles, instead, as both *B. hypnorum* and *B. terrestris* sometimes do, on the naked capsules, after the blossoms have fallen, over the surface of which it passes its proboscis. For some time I thought that the habits of this bee were invariable, since I never saw it either enter the cup of a foxglove, or descend upon any part of the corolla, which it simply passed by. One day, however, I found what appeared to me to be an individual of this species inside a foxglove, in a drowsy or lethargic condition, such as often overtakes Humble-Bees. On taking it, out it fell to the ground, but, recovering, shortly, flew off, and went directly to another blossom, on which it alighted, and crawling to the base of the tube, outside, probed it through a hole which I have now no doubt that it found there. For some time after this I followed its movements, and saw that it now always went to the naked ovaries, instead of either entering or probing the cups, nor did it again alight on one of these. From this, coupled with its appearance, I believe it to have been the same small brown species that I had always before seen acting in this manner, and never getting into the foxgloves. Why, then, was it in one now? But for this apparent recovery one might suppose that it had crawled there to die, and it would be interesting to know how long afterward it really lived. Many bees, at this time, seemed, in appearance, near to death, whilst others had died actually, as the natural termination, apparently, of the same lethargic state. Bees, however, can have no idea of death, a matter not within the grasp of their intelligence, so that it would not be that, but the state of health

preceding it, which caused this bee to enter a foxglove against its usual habits, if there was really a connection between the two facts.

I made a similar observation in regard to *B. terrestris*, a species which, whatever are its habits in England, may be watched here, hour after hour and day after day, without ever being seen to enter the cup of a foxglove—always either the perforated necks of the flower or the naked green calyces are resorted to. This particular individual, however, when first observed by me, was just crawling, in a state, as it seemed, of great decrepitude, into one of the "gloves." With the view of, as far as possible, testing its object in doing so, I took out my scissors, and snipped off a portion of the tube, longitudinally. Almost immediately I saw the proboscis of the bee shoot out, to an astonishing length, over the moist surface of the calyx thus laid bare. This was a wonderful thing to look at through the Coddington lens, which I could do now with perfect ease. The proboscis was very long, and when it seemed that it could stretch no farther, another and thinner portion darted out from what had seemed the end of it, the tip of which was enlarged and tripartite, having, as it were, three lips, which pressed upon the exposed surface of the pistil or ovary of the flower. It then shot back, and this process was repeated, at intervals, two or three times, the instrument being, no doubt, employed, when I did not see it, in searching some part of the calyx that had not been laid bare. The bee, now, slowly and with great difficulty—in the most decrepit manner imaginable—crawled out of the foxglove, over another, and into the one next it, where, again, upon using the scissors, I saw the proboscis at work. Then, coming out once more, it just managed to get on to the mouth of another blossom—a short one—where it clung, seeming to be on the point of death.

Here, then, we have two instances of bees, not ordinarily in the habit of entering foxgloves, doing so whilst in a state which, whether it precedes death or not, is not, at any rate, a normal one. In one of these cases, however, and therefore, presumably, in the other also, not only has the bee entered the cups, but, as we have seen, it has crawled up to their ends, and extracted the juices of the flower, as do those who habitually obtain them in

this way. It would seem, therefore, that there may be a curious change in a bee's life-habits, consequent upon the approach of a lethargy which may or may not precede death. The primary instinct or habit, however—that of extracting nectar from the flower—remains unmodified, and supposing that this last bee really was dying, then, so far from feeling or providing for its approaching dissolution, we see it continuing at its work as long as ever its strength will allow it to, and expending its last energy either in rifling the flower it is in, or endeavouring to get to another, in order to do so—a strong instance of “the ruling passion” being strong in death.

But why should there, now, be a change in the method of rifling the flower? Although, as I have said, one may watch bees that habitually do not enter foxgloves, for a very long time, and for many days in succession, without seeing them do so, yet it seems reasonable to conclude that this more obvious process, which allows of every flower being ransacked, before its corolla has either been shed or perforated, was the first one employed by all species, and that the others represent departures from it. If this be so, then it would appear that the lethargy, however induced, under which a bee whose individual habits have thus come to differ from its ancestral ones, is labouring, produces a mental disturbance which, in some cases, may take the form of a reversion to these earlier habits. If so, then we have here a principle through which light might be thrown upon the course of evolution, not only in bees, but also in some other insects, or species, belonging to other divisions of the animal kingdom.

I subsequently introduced a lethargic bee belonging to one of the species, which does not habitually enter the flowers of the foxglove, into one, on which she crawled painfully up it, and on my cutting the base of the tube with the scissors, as before, I saw her proboscis several times shot out, as in the former case. She then came out, and I put her into several others, which she each time vacated, and then, seeming to take a new lease of life and energy, whirled her wings, and flew away. Watching her movements, however, I saw that there was something peculiar about them. She flew in an aimless and, as it were, confused sort of way, mostly in circles, and faster than usual. In this

manner she buzzed round some of the foxgloves, but without alighting or slackening speed, to alight, then made a wide circuit or two; high up, and, at last, flew right into the fir forest surrounding these open spaces, which I have never before, if I remember, seen a bee here do.

Thus it seems clear that, with the coming on of this drowsiness, the psychology of the bee is affected, and though we may not exactly see why, yet it is not inconceivable that such mental disturbal may produce a reversion to past ancestral habits, in which category entering the cup of the foxglove, in order to extract the nectar, would fall, in the case of a bee that was not accustomed to do this, if we suppose that such entry was the primitive method adopted, and that the others of probing the tube from without, or visiting those flowers only that had shed their corollas, were deviations from it, subsequently arising. In illness, and also in old age, the mind is often filled with the memories of childhood, and though the reversion here is only to one's past, still it is a reversion, and may be governed by the same laws as obtain in the other. Drowning, again, is apparently attended by the same phenomenon. I am assuming, of course, that the bee's individual habits have always been the same. Otherwise, the analogy offered by the above cases would be much closer, if not exact.

As the bees do not either bite through the neck of the foxglove with their mandibles, or pierce it with their proboscis, to what agency are the holes which they find ready-made there attributable? On several occasions I had noticed a small Longicorn, or Longicorn-like beetle, in this situation, and I thought, though I could not be quite sure, that one of these was biting at the neck of the foxglove, inside which he was. Longicorns, at any rate, are, I believe, vegetarians, and as this one seems to live largely on the foxglove, it is probable that it does so in a double sense. Though small, this beetle is not so very small—as large, perhaps, though the shape is different, as the house-fly and there is at least one larger species whose habits appear to be the same. Through the lens, the mandibles of both look very well adapted for making these little holes in the walls of flowers. They are long and sharply pointed, finely though

strongly made, and somewhat sickle-shaped. Thus, then, supposing these beetles to be the makers of the holes in question, we have, at least, three species of Humble-Bee taking advantage of their handiwork to insert their proboscis through the basal part of the corolla of the foxglove, from without, instead of entering it, which it does not appear to be their habit to do.

Assuming that the ancestors of those bees that do not now enter the foxglove flowers, in order to rifle them, were in the habit of doing so, what, if any, has been the gain to the species, through which this change of habit has been brought about? Saving of time is the only one that I can imagine, and certainly a bee that descends directly on those parts of the flower where the juices which she covets reside, can sooner obtain them than one who comes down farther off, by the length of the long tunnel, formed by the corolla, up which she has first to climb. But, on the other hand, a bee which flies from one such tunnel to another, looking for holes in them, through which it can thrust its proboscis, which holes it does not always find, would seem to be losing time; yet this is what I have seen many bees doing. Here it would depend on how numerous such holes were, and, in regard to this, they must have been fairly numerous, one would think, for such a habit to have arisen at all. Still, though, here and there, almost every foxglove seemed perforated, in this way, over any large area, they formed, I believe, but a small minority. Possibly the bee may be aided here by its eyesight, yet it was common for them to settle on the necks of unperforated tubes, from which they had to fly, bootless, away. These bees certainly lost time, but they might, perhaps, more than make up for this by a succession of successful alightments, of which I also saw many instances.

Bees that search the foxgloves in this way, rifle, also, those flowers which have lost their corollas, yet I have seen individuals going so continuously from tube to tube, to probe them from without, that one would not have supposed that they did anything else, and this was particularly the case with one species, the small black Humble-Bee, with a yellow-tipped abdomen—*B. mort-nucatus* namely—which I have mentioned. I am not, indeed, quite sure that the latter does not feed exclusively in this manner

—I mean, of course, when visiting the foxglove. Even if we suppose this bee to be very quick in noticing these small perforations in the neck of the corolla—which would not, however, look so small to it—yet it has to miss a number of flowers, whereas the bee who enters them can rifle every one. *B. terrestris*, also, though alighting sometimes on the naked calyx, yet certainly, through the same cause, misses a number of blossoms. It would seem, therefore, that the change from the orthodox way, as we may call it, of obtaining nectar from the foxglove, to the ones we are considering, must represent a loss rather than a gain of time, and this should make us doubt whether any such change has taken place. Of course, if the proboscis of any of these bees were not sufficiently long to be effectually employed from within the tube, the whole philosophy of the matter would be changed, and the possibility of any such evolution, as is here imagined, be excluded, in their case. But how can this be? The part of the foxglove which has to be reached is the moist green base, more or less swollen, of the pistil, and this does not appear to be so tightly enclosed within the tube of the corolla but that a bee, whose proboscis was not altogether abnormal, might press up, so as to reach it, without undue difficulty. Both *B. mastrucatus* and the small brown bee might, I think, very well do this, and it is probably what that individual of the latter species—the smaller of the two—that I found in one of the “gloves” was doing. *B. terrestris*, in any case, which here rarely enters the corolla, but either probes it from without or licks the corolla-less pistils, can, as has been seen, with the greatest ease, put its proboscis to a like use within the tube. Yet, in spite of its being under no physical disability of rifling the foxglove in the ordinary manner (as in England), and though it does occasionally do so, yet this bee, where I have watched it, in the Black Forest, habitually obtains the nectar through perforations that have been previously made in the corolla, passing by such as are not thus perforated. The presumption, I think, is that it has changed its earlier habits in this respect, and, if so, this is probably also the case with the two smaller kinds. Must we therefore conclude that the change has been beneficial to the species? This does not appear to me to be a necessary inference, and, were foxgloves the only flowers, one might rather

suppose the contrary, since the two species that search them, most constantly, in the regular manner, are much more numerous, where they abound, than those whose habits have been modified. If no conclusion can be drawn from this circumstance, yet I am unable to see what gain can accrue, from such a change, to the species, though it may mean less trouble to the individual. But nations that have become effete on this principle have not disappeared at once, and there should be ample time to observe the deleterious variations in the habits of a species, before these have cost it its life.

The above observations were made by me from August 18th to 26th, and were confined to a particular patch of foxgloves in that part of the Black Forest where I was staying. From some earlier ones made in other and much smaller patches, it has occurred to me that the flower-searching habits of the same species of *Bombus* may differ locally, by which I mean in places only a short distance apart. As the worker bees do not go a very great way from the nest, and as the fertilized queen probably does not do so either, this is not, in itself, less improbable than that different dialects of a language—*e. g.* Norwegian—should have grown up in valleys quite near to, but cut off, by high intervening mountains, from, one another. In neither case can the inhabitants of neighbouring districts intermix, which is the condition above all requisite for divergence both of habit and speech. Since, however, my previous observations were made, casually, when my mind was occupied with another subject, and were not noted down at the time, I only mention this as a matter of possibility, which it might be worth while to investigate.

I do not recall having ever, in England, seen a Humble-Bee obtaining the nectar of the foxglove otherwise than by entering the flower—but foxgloves are not common in England. As Darwin, however, mentions bees being sometimes in such a hurry to rifle flowers as to bite holes through their corollas, I will here once more say that, to the best of my observation and belief, these bees of the Schwarzwald never did so whilst searching the foxglove beds. Not only did they leave such flowers as were not already perforated, but such perforations as they utilized, showed, by their discoloured edges, that they had not

been made by themselves.* This, of course, would not exclude the agency of a previous bee, but why should one individual depend on another for what it could equally well do itself? Moreover, the biting of a hole, by a bee, in any flower that it can reach by entry, would appear to be a very doubtful method of saving time. A previously perforated foxglove would, however, enable it to save trouble, and in this we probably have the real motive of action. By counting the number of foxglove flowers searched, in a given time, by representatives of each method, it would be possible, perhaps, to find out whether this saving of trouble is synonymous with saving of time. Should it, however, appear that the non-foxglove-entering bees worked less quickly than the others, this would not quite settle the question, since the factors of duration of labour and amount of rest required would still remain to be considered. To wedge itself up one narrow tube, after another, must certainly be greater labour for a bee than flight between flower and flower; greater labour must require a greater amount of relaxation from it, and I have seen Humble-Bees, which were not in a lethargic condition, sitting, for some while, motionless, as though resting.

* As bearing on this question, I may mention that various Humble-Bees that I confined inside foxgloves, by tying up the mouth with cotton, remained prisoners, for a long time, before they began to bite the corollas in order to force their way out, which was such a labour to them that some on emerging lay, for a time, motionless, as if exhausted. This may not *prove* that it is not their custom to bite through foxgloves, from without, but it does not favour that view. There would, however, be nothing extraordinary in the fact of bees that once bit their way into foxgloves having now become dependent on the work of other insects, in this respect. Ants, now fed by slave ants, once fed themselves, and can still do so to some extent, and (if I am not mistaken) in differing degrees. In this connection the facts here recorded become all the more interesting.

NOTES FROM YORKSHIRE.

By E. P. BUTTERFIELD.

QUITE recently Mr. J. W. Carter, F.E.S., of Bradford, sent me a few dead bees (*Bombus* sp.) which he had picked up beneath the blossoms of some lime-trees in Patterdale, in the Lake District, all of which had neat holes in the thorax and abdomen, from which the contents had been abstracted. About a year ago a gamekeeper told me he had witnessed a similar occurrence under an avenue of lime-trees in this district; all the bees which he examined had apparently met with their death in a similar manner to the specimens sent by Mr. Carter.

The late Mr. James Varley, of Huddersfield, recorded a similar phenomenon in the 'Naturalist,' vol. iii. p. 40. He mentions having found hundreds of dead bees under lime-trees on his way to Woodsome. These were sent to the late Mr. Frederick Smith, of the British Museum, and he suggested the probability of their having met with their death by the Red-backed Shrike, which seems to have a partiality for bees.

The Red-backed Shrike is what might be considered practically absent from this district, and so cannot be responsible for the cause of the deaths mentioned by the gamekeeper referred to above. The more probable culprit, I think, will turn out to be one of the Tit family, probably either the Blue or Great Tit, both of which are found in abundance in this district.

There should be no great difficulty in ascertaining the cause of such havoc among bees; that it is due to some species of bird or birds I have little doubt, although it has been suggested that dead bees found under lime-blossoms might have been poisoned, and the perforation in their bodies been due to ants, &c.*

A friend of mine near Keighley has been wanting me now for some time to pay a visit to a Starling roost near his residence.

* Six specimens of *Bombus lucorum* from Gloucestershire were sent by Dr. Günther to the British Museum for identification. They had been taking honey from *Tilia petiolaria*, the flowers of which attract them, and, having apparently become stupefied, they had been attacked by wasps, which had made holes in the thorax.—ED.

He informs me that every evening, not only thousands but actually millions assemble, and have done for some time. Soon after the young left their nest this season, I saw the largest flock it has ever been my privilege to witness. Probably no British bird has multiplied so rapidly within recent years as this species, and wherever I have visited within a radius of ten or twenty miles of this village (Wilsden) it is found to be chiefly single-brooded. This is easily ascertainable where it breeds in colonies. The Siberian form predominates here, and it would be interesting to ascertain where this species is said to be double-brooded, and whether it is the old English form. For some reason there has been this year a relatively larger proportion of late broods. One reason may be: they began to breed somewhat earlier owing to the fine spell of weather in March. This might have induced a few to attempt a second brood, but I should think most of the late broods had their first nests destroyed.

On or about August 12th two boys told me they had found a Snipe's nest the previous day in a situation the least likely for this species of any in the district. On account of the late date and unlikely place for the nest of this bird, I thought the boys must be mistaken. However, my informants were quite right. I found it to be the nest of a Snipe with one egg in an advanced stage of incubation. The old bird flushed off when only within a few feet from its nest. Probably it had had its first, and possibly its second, nest destroyed.

Of late years hereabouts a shrub (*Daphne*) has been much cultivated by gardeners, and its berries have a great attraction for Greenfinches. They eat the seeds only, rejecting the pulpy mass, and these birds, which are so shy at other times, will come into our main streets and feed upon these berries within a few feet of passengers. I have never seen any other species of birds feeding upon these berries. This partiality of birds for certain berries and other fruits is an interesting question. The Bullfinch is a very rare breeding species in this neighbourhood, but when the elderberries are ripe I can always count upon seeing it. Ring-Ouzels are very fond of bilberries, and also very partial to the berries of the mountain-ash, and come from the moors in some numbers when these berries are ripe. Starlings and Mistle-Thrushes are both fond of rowan-berries.

NOTES AND QUERIES.

AVES.

Nightingale and Willow-Wren in Captivity. — I had recently a great treat, being taken by a friend to see a collection of foreign and British birds kept by a German working man in a small attic over his workroom in a house in the town, and was surprised to see all the birds in such perfect health and plumage, for they had only a minimum of light and scarcely any sunshine. I was particularly interested in a Nightingale kept for three years, and also a Willow-Wren, lively and active, hopping about as if in its native haunts, and so tame that both took wasp-grubs from the hand. There were also a large number of rare and valuable foreign birds, all in fine plumage and health, a pair of Hoopoes, and a pair of Grey Wagtails. —ROBERT WARREN (Ardnaree, Monkstown, Co. Cork).

Albino House-Sparrow in Yorkshire. — On August 24th last Miss Grimshaw, of Eden Place, Ackworth, in the West Riding of Yorkshire, showed me a beautiful example of an albino House-Sparrow (*Passer domesticus*) which had just previously been killed by her cat. There was no colouring matter whatever in the plumage, tarsi, toes, claws, or beak, which were pure white. The carcase was fortunately not damaged, and the bird was sent to Mr. Cullingford, of Durham, to preserve, and on dissection it was found to be a male. —WALTER B. ARUNDEL (High Ackworth, Pontefract).

Late Eggs of Nightjar (*Caprimulgus europæus*): Was it a Second Brood? — The present abnormal summer, with its autumn-like days and nights, may account for almost any irregularity we may observe in the economy or occurrence of birds, insects, or plants, and I am aware that the occasional double-broodedness of this peculiar summer-loving bird is an open question; consequently the following note may be of interest: — On Aug. 15th I had two eggs sent me, which had been picked up the day previously on exactly the same spot where a pair of birds had been hatched and reared in the early part of the season — I think in June. The two eggs in question were perfectly fresh, the yolks were intact, and without the least indication

of having been incubated, although one of the birds—presumably the female—rose from the spot when approached. One of the eggs was of a generally lighter colour, from the fact of the two shades of markings being very much paler than usual, as if the parent had lacked the full amount of colouring pigment. Of course, I am not at all sure that the two young birds and the eggs belonged to the same parents, but I believe it is often observed that, like others of the Swallow kind, this species, if undisturbed, will return more than once to the same nesting-place, and, as the former hatching proved successful, a second brood may have been anticipated from the same quarters; and, on the other hand, it may have been only a coincidence, in which two pairs of birds chose the same site for their home; but in either case it seems to me somewhat strange that a bird should attempt to rear a family so near to its departure to a warmer clime, where, if observation is correct, they neither marry nor are given in marriage, a forcible proof that they love the land of their nativity, though they wander far. From what I heard, the birds have been seen in some numbers during the past season, and their “gurglings” were very frequent, notwithstanding the chilly evenings. With regard to late broods of migrating species, we know that the instinct of migration is so strong in the House-Martin that a nest of late young is sometimes left to starve, if, indeed, the supposition of neglect is correct. Is it not possible in such a case that the parents themselves have succumbed to starvation, or have been ruthlessly slain?—G. B. CORBIN (Ringwood, Hants).

A Variety of the Gannet (*Sula bassana*).—Upon a recent visit to the Bass Rock, I saw a very interesting and handsome variety of the Gannet. The whole of the head and neck was of a rich dark buff colour, the back thickly mottled with large crescent-shaped markings of the same rich colour, and the wings were mottled with spots, though not so large or so dark as those on the back. The primaries, feet, legs, beak, and eyes were of normal colour. The bird was mature, and had mated with one of the normal colour, and both were mounting guard over their solitary young one. I obtained several photographs of the bird. Out of the many thousands of birds frequenting the Rock, this was the only one I saw which departed in any way from the normal.—R. FORTUNE (5, Grosvenor Terrace, East Parade, Harrogate).

Early Building of Herons.—In some seasons Herons begin building very early. In 1896 they began building in a small wood at Moy View, Co. Sligo, on January 15th, several pairs were hatching on

February 1st, by the end of the month the young in the nests were heard calling loudly and strongly for food, and by February 8th all in that wood were apparently hatched. They generally begin building in that locality in February, but January 15th was the earliest date that has come under my notice since the birds came to the wood over forty years ago. — ROBERT WARREN (Ardnaree, Monkstown, Co. Cork).

Herons breeding twice in the Season.—For many years, seeing very young Herons in July and August, I was puzzled as to whether these birds really reared two broods, or whether the late young birds were the produce of parents that had lost their first clutch of eggs or young by the nests being blown down during the March storms. However, in May, 1896, my doubts were cleared. Within sixty yards of Moy View Cottage, in the spring of 1896, a pair of Herons built a nest in a tree alongside the path leading from the house to the shore, and were daily under our notice while hatching and rearing their young; these were fully fledged by the end of April. On May 7th we observed the old birds beginning to build a second nest in a fir-tree in the garden about thirty yards from a bedroom window, but the second day I was attracted by a great noise, as if the birds were scolding or fighting. However, on going out to the garden, I found that the young birds had followed the old ones to where they were at the new nest, and the uproar was caused by the young ones persistently following the old birds and calling for food, and by the old birds scolding and driving the young ones away from the new nest. So here the doubts as to a second brood were solved by seeing the young of the first nest following and annoying their parents by clamouring for food when they should have been feeding themselves. — ROBERT WARREN (Ardnaree, Monkstown, Co. Cork).

Correction.—Mr. Owen wishes to make a correction to his recent communication, "An Account of a Ramble with the Birds in Anglesey and Carnarvonshire" (*ante*, p. 310). For "Anglesey" (top line, p. 311) substitute "a small village in Carnarvonshire."

NOTICES OF NEW BOOKS.

The Subantarctic Islands of New Zealand; Reports on the Geophysics, Geology, Zoology, and Botany, &c. Edited by CHARLES CHILTON, M.A., D.Sc., &c. Published by the Philosophical Institute of Canterbury, Wellington, N.Z. London: Dulau & Co., Ltd.

THE scientifically unexplored islands of the world are becoming fewer, and the surface of the planet on which we live is rapidly losing its secrets so far as fauna and flora are concerned. The islands which have afforded the subject-matter for these two truly biological volumes were till recently better known as spots visited by whalers, or the inhospitable scenes of not a few shipwrecks; owing, however, to the enterprise and incitement of the Philosophical Institute of Canterbury, and the wisdom of the New Zealand Government, they have now been included in our ever-increasing faunistic records by the work of a scientific party landed on the Auckland and Campbell Islands during the annual trip of the Government steamer 'Hinemoa' in November, 1907. —

This publication is a very thorough production, and an ample historical Introduction is given, including "The Discovery of the Islands," by the Hon. R. McNab, and a detailed account of "The Subantarctic Islands of New Zealand and the History of their Scientific Investigation," by Dr. Charles Chilton, the latter contribution being fully illustrated and intensely readable. As may be expected, the subject-matter of the two volumes is the work of specialists, and is descriptive of the material collected during the expedition. The insects collected by the Campbell Island party were mostly Coleoptera and Diptera, and we are told by Mr. Hudson that, "owing to the prevailing heavy winds, the insects at Campbell Island fly very little, and unless they are captured whilst at rest on some plant it is

almost impossible to net them, as the wind picks them up the moment they leave the flower, and whirls them away some ten or twenty feet." Major T. Broun, who has worked out the Coleoptera, has formed the following conclusion:—"Assuming that a considerable area of land formerly extended from the Auckland Islands towards Patagonia, the New Zealand Islands must have formed a portion of it." Mr. H. R. Hogg, from a study of the *Arachnidæ*, has formed a similar opinion:—"The supposition of an ancient land-link between South America, Australia, and Southern Africa is more or less of a necessity in order to account for the present distribution of creatures which it is difficult to believe could have reached their respective habitats by any other means."

Mr. E. R. Waite has dealt with the vertebrates. "There are no reptiles on the islands." The mammalian fauna is small, and represented by "species of cetaceans, by two kinds of resident Seals, and occasional visitors or stragglers of the order." The account of the birds is stated to be very inadequate for several reasons, one of which was a rule of the expedition that neither birds nor their eggs were to be taken. The Albatrosses *Diomedea exulans* and *D. regia* and the Mollymawk (*D. melanophrys*) breed on the islands, and some fine photographs of these birds and their nests are given. The "Flightless Duck" (*Nesonetta aucklandica*) is rather misnamed, as, according to Capt. Bollons, "these ducks are able to fly for short distances, and, as a matter of fact, they reach their nesting-sites by this means." The most interesting discussion in the description of the fishes is the disinclination of Mr. Waite to accept *Galaxias brevipinnis* as a marine species, as it is considered by some very high authorities. Dr. Chilton has fully enumerated and described the Crustacea. One interesting fact in this communication relates to the genus *Parorchestia*. The male of *P. sylvicola* on the main islands of New Zealand is very rare, nearly all the specimens captured being females; yet in the three species of the genus found on the Auckland and Campbell Islands the males appear to be almost as abundant as the females.

The botanical and geological sections do not appertain to our pages, and we have been unable to refer to the contributions

of all the specialists in these volumes. Enough, however, has surely been noticed to prove the importance to zoologists of the results of this somewhat short but important expedition.

Life of William Macgillivray. By WILLIAM MACGILLIVRAY, with a scientific appreciation by Prof. J. ARTHUR THOMSON. John Murray.

It is well that we should know more of the life of this devoted ornithologist, well described by Darwin as "the accurate Macgillivray," and the first half of the volume which is devoted to biographical details gives us all the principal events comprised in a busy life, even if it does not present the personal characteristics that lift a biography into a human document. We can, however, glean much of the man himself in the narrative of his work; his could have been no nebulous personality to have drawn to his lectures so fine a judge of style and matter in other fields as the late Prof. Blackie. Besides attempting to found a permanent classification of birds on structural characters, he anticipated our modern bird-watchers. "Much of his holiday time was spent in watching, by night as well as by day, the habits of birds, and he often concealed himself for many hours continuously, now in some cave or rocky recess by the shore, from which the variety of swimming birds could be most readily seen, and again in some temporary shelter erected on the higher cliffs, from which the Eagle, the Osprey, the Raven, and other predatory birds could be closely observed." His walk from Aberdeen to London in order to see the British Museum and other kindred institutions is a narrative of Scottish frugality and endurance adorned by natural reflections and appreciations of events and scenery which come not to every pedestrian.

Besides being an ornithologist, Prof. Thomson, in his appreciation, acutely points out that Macgillivray was one of that now almost extinct type—the all-round naturalist—that he was a well-equipped geologist, botanist, and zoologist, and that "he taught all the three sciences with conspicuous success." These qualities must have made him appreciate the wide intellectual purview of Alexander von Humboldt, whose published travels and researches he condensed, a memorable classic, containing some

mistaken conclusions based on imperfect data but not on ignorance of the knowledge of the day. Humboldt's encyclopædic attainments must have won the admiration of Macgillivray.

Eight illustrations of birds drawn by Macgillivray, and now contained in the British Museum, are reproduced in this volume, and add to its attraction. Misprints appear to be few, though in the preface we notice that Mr. Pycraft has had an extra vowel added to his name. We have also been somewhat in doubt as to the proper way to write the name of this great British ornithologist. On the title-page it appears twice as "Macgillivray"; throughout the volume it is written "MacGillivray." Rightly or wrongly, we have followed the title-page.

Faune des Mammifères d'Europe. Par E.-L. TROUESSART.
Berlin: R. Friedländer & Sohn.

IN his preface Prof. Trouessart compares the evolutionary views of to-day with those of the immutability of species at the time (1857) when Blasius published his 'Naturgeschichte der Säugethiere Deutschlands und der angrenzenden Länder von Mitteleuropa.' At that time Blasius followed the doctrine of Cuvier; to-day, in a similar undertaking, Trouessart writes as a disciple of Darwin. Four principal divisions are recognized in this fauna:—(1) "La faune de l'Europe Centrale, la moins caractérisée de toutes, attendu qu'elle ne présente que les espèces vulgaires, généralement répandues sur tout le Continent." (2) "La faune Arctique caractérisée par *Ursus maritimus*, *Canis lagopus*, *Gulo borealis*, *Lepus timidus* (ou *variabilis*), *Rangifer tarandus*, *Alce alces*, &c. À l'Epoque Glaciaire cette faune s'est avancée jusqu'aux Pyrénées." (3) "La faune des Steppes Asiatiques, caractérisée surtout par ses Rongeurs des genres *Citellus*, *Gerbillus*, *Cricetus*, *Cricetulus*, *Spalax*, *Dipodipus*, *Alactaga*, *Ochotona*, &c.; cette faune, que vit encore dans le Sud-Est de la Russie, s'est avancée jusque dans le centre de l'Europe pendant la période de sécheresse qui succède à l'Epoque Glaciaire, et y a laissé des survivants, par exemple, *Cricetus cricetus* (le Hamster)." (4) "Enfin la faune Africaine ou Méditerranéenne, caractérisée par *Genetta vulgaris*, *Herpestes ichneumon*, *Canis*

aureus, *Felis ocreata*, *Hystrix cristata*, *Lepus mediterraneus*, &c., semble un résidu de la faune de l'Epoque Tertiaire."

In the treatment of species a binomial and analytical method is employed. The genus *Mus* is liberally treated, though *M. flavicollis*, Melch., is not considered as distinct from *M. sylvaticus*, as recently advocated by a writer in these pages. But all these different representatives of *Mus*, whether regarded as species, varietal or geographical forms, are clearly diagnosed and their localities detailed. Difference of view on these questions seems to be as clearly found among mammalogists as among other zoological specialists, and will probably continue as the classificatory pendulum sways between the analytic and synthetic foundations.

Prof. Trouessart's volume will sustain the reputation of its writer; it is published at a time when we believe other works of a similar or somewhat similar character will also shortly appear.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, SHEFFIELD, 1910.

ADDRESS TO THE ZOOLOGICAL SECTION.

By Professor G. C. BOURNE, M.A., D.Sc., F.R.S., *President of the Section.*

IN choosing a subject for the address with which it is my duty, as President of this Section, to trouble you, I have found myself in no small embarrassment. As one whose business it is to lecture and give instruction in the details of comparative anatomy, and whose published work, *qualecunque sit*, has been indited on typical and, as men would now say, old-fashioned morphological lines, I seem to stand self-condemned as a morphologist. For morphology, if I read the signs of the times aright, is no longer in favour in this country, and among a section of the zoological world has almost fallen into disgrace. At all events, I have been very frankly assured that this is the case by a large proportion of the young gentlemen whom it has been my fate to examine during the past two years; and, as this seems to be the opinion of the rising generation of English zoologists, and as there are evident signs that their opinion is backed by an influential section of their elders, I have thought that it might be of some interest, and perhaps of some use, if I took this opportunity of offering an apology for animal morphology.

It is a sound rule to begin with a definition of terms, so I will first try to give a short answer to the question, "What is morphology?" and, when I have given a somewhat dogmatic answer, I will try to deal in the course of this address with two further questions: What has morphology done for zoological science in the past? What remains for morphology to do in the future?

To begin with, then, what do we include under the term morphology? I must, first of all, protest against the frequent assumption that we are bound by the definitions of C. F. Wolff or Goethe, or even of Haeckel, and that we may not enlarge the limits of morphological study beyond those laid down by the fathers of this branch of our science. We are not—at all events, we should not be—bound by authority, and we owe no allegiance other than what reason commends to causes and principles enunciated by our predecessors, however eminent they may have been.

The term morphology, stripped of all the theoretical conceptions that have clustered around it, means nothing more than the study of form, and it is applicable to all branches of zoology in which the relationships of animals are determined by reference to their form and structure. Morphology, therefore, extends its sway not only over the comparative anatomy of adult and recent animals, but also over palæontology, comparative embryology, systematic zoology and cytology, for all these branches of our science are occupied with the

study of form. And in treating of form they have all, since the acceptance of the doctrine of descent with modification, made use of the same guiding principle—namely, that likeness of form is the index to blood-relationship. It was the introduction of this principle that revolutionized the methods of morphology fifty years ago, and stimulated that vast output of morphological work which some persons, erroneously as I think, regard as a departure from the line of progress indicated by Darwin.

We may now ask, What has morphology done for the advancement of zoological science since the publication of the 'Origin of Species'? We need not stop to inquire what facts it has accumulated: it is sufficiently obvious that it has added enormously to our stock of concrete knowledge. We have rather to ask, What great general principles has it established on so secure a basis that they meet with universal acceptance at the hands of competent zoologists?

It has doubtless been the object of morphology during the past half-century to illustrate and confirm the Darwinian theory. How far has it been successful? To answer this question we have to be sure of what we mean when we speak of the Darwinian theory. I think that we mean at least two things. (1) That the assemblage of animal forms as we now see them, with all their diversities of form, habit, and structure, is directly descended from a precedent and somewhat different assemblage, and these in turn from a precedent and more different assemblage, and so on down to remote periods of geological time. Further, that throughout all these periods inheritance combined with changeability of structure have been the factors operative in producing the differences between the successive assemblages. (2) That the modifications of form which this theory of evolution implies have been rejected or preserved and accumulated by the action of Natural Selection.

As regards the first of these propositions, I think there can be no doubt that morphology has done great service in establishing our belief on a secure basis. The transmutation of animal forms in past time cannot be proved directly; it can only be shown that, as a theory, it has a much higher degree of probability than any other that can be brought forward, and in order to establish the highest possible degree of probability, it was necessary to demonstrate that all anatomical, embryological, and palæontological facts were consistent with it. We are apt to forget, nowadays, that there is no *a priori* reason for regarding the resemblances and differences that we observe in organic forms as something different in kind from the analogous series of resemblances and differences that obtain in inanimate objects. This was clearly pointed out by Fleeming Jenkin in a very able and much-referred to article in the 'North British Review' for June, 1867, and his argument from the *a priori* standpoint has as much force to-day as when it was written forty-three years ago. But it has lost almost all its force through the arguments *a posteriori* supplied by morphological science. Our belief in the transmutation of animal organization in past time is founded very largely upon our minute and intimate knowledge of the manifold relations of structural form that obtain among adult animals; on our

precise knowledge of the steps by which these adult relations are established during the development of different kinds of animals; on our constantly increasing knowledge of the succession of animal forms in past time; and, generally, on the conviction that all the diverse forms of tissues, organs, and entire animals are but the expression of an infinite number of variations of a single theme, that theme being cell-division, multiplication, and differentiation. This conviction grew but slowly in men's minds. It was opposed to the cherished beliefs of centuries, and morphology rendered a necessary service when it spent all those years which have been described as "years in the wilderness" in accumulating such a mass of circumstantial evidence in favour of an evolutionary explanation of the order of animate nature as to place the doctrine of descent with modification on a secure foundation of fact. I do not believe that this foundation could have been so securely laid in any other way, and I hold that zoologists were actuated by a sound instinct in working so largely on morphological lines for forty years after Darwin wrote. For there was a large mass of fact and theory to be remodelled and brought into harmony with the new ideas, and a still larger vein of undiscovered fact to explore. The matter was difficult and the pace could not be forced. Morphology, therefore, deserves the credit of having done well in the past: the question remains, What can it do in the future?

It is evident, I think, that it cannot do much in the way of adding new truths and general principles to zoological science, nor even much more that is useful in the verification of established principles, without enlarging its scope and methods. Hitherto—or, at any rate, until very recently—it has accepted certain guiding principles on faith, and, without inquiring too closely into their validity, has occupied itself with showing that, on the assumption that these principles are true, the phenomena of animal structure, development, and succession receive a reasonable explanation.

We have seen that the fundamental principles relied upon during the last fifty years have been inheritance and variation. In every inference drawn from the comparison of one kind of animal structure with another, the morphologist founds himself on the assumption that different degrees of similitude correspond more or less closely to degrees of blood-relationship, and to-day there are probably few persons who doubt that this assumption is valid. But we must not forget that, before the publication of the 'Origin of Species,' it was rejected by the most influential zoologists as an idle speculation, and that it is imperilled by Mendelian experiments showing that characters may be split up and reunited in different combinations in the course of a few generations. We do not doubt the importance of the principle of inheritance, but we are not quite so sure as we were that close resemblances are due to close kinship and remoter resemblances to remoter kinship.

The principle of variation asserts that like does not beget exactly like, but something more or less different. For a long time morphologists did not inquire too closely into the question how these differences arose. They simply accepted it as a fact that they occur,

and that they are of sufficient frequency and magnitude, and that a sufficient proportion of them lead in such directions that natural selection can take advantage of them. Difficulties and objections were raised, but morphology on the whole took little heed of them. Remaining steadfast in its adherence to the principles laid down by Darwin, it contented itself with piling up circumstantial evidence, and met objection and criticism with an ingenious apologetic. In brief, its labours have consisted in bringing fresh instances, and especially such instances as seemed unconformable, under the rules, and in perfecting a system of classification in illustration of the rules. It is obvious, however, that, although this kind of study is both useful and indispensable at a certain stage of scientific progress, it does not help us to form new rules, and fails altogether if the old rules are seriously called into question.

As a matter of fact, admitting that the old rules are valid, it has become increasingly evident that they are not sufficient. Until a few years ago morphologists were open to the reproach that, while they studied form in all its variety and detail, they occupied themselves too little—if, indeed, they could be said to occupy themselves at all—with the question of how form is produced, and how, when certain forms are established, they are caused to undergo change and give rise to fresh forms. As Klebs has pointed out, the forms of animals and plants were regarded as the expression of their inscrutable inner nature, and the stages passed through in the development of the individual were represented as the outcome of purely internal and hidden laws. This defect seems to have been more distinctly realised by botanical than by zoological morphologists, for Hofmeister, as long ago as 1868, wrote that the most pressing and immediate aim of the investigator was to discover to what extent external forces acting on the organism are of importance in determining its form.

If morphology was to be anything more than a descriptive science, if it was to progress any further in the discovery of the relations of cause and effect, it was clear that it must alter its methods and follow the course indicated by Hofmeister. And I submit that an inquiry into the causes which produce alteration of form is as much the province of, and is as fitly called, morphology as, let us say, a discussion of the significance of the patterns of the molar teeth of mammals or a disputation about the origin of the coelomic cavities of vertebrates and invertebrates animals.

There remains, therefore, a large field for morphology to explore. Exploration has begun from several sides, and in some quarters has made substantial progress. It will be of interest to consider how much progress has been made along certain lines of research—we cannot now follow all the lines—and to forecast, if possible, the direction that this pioneer work will give to the morphology of the future.

I am not aware that morphologists have, until quite recently, had any very clear concept of what may be expected to underlie form and structure. Dealing, as they have dealt, almost exclusively with things that can be seen or rendered visible by the microscope, they have acquired the habit of thinking of the organism as made up of organs, the organs of tissues, the tissues of cells, and the cells as

made up—of what? Of vital units of a lower order, as several very distinguished biologists would have us believe; of physiological units, of micellæ, of determinants and biophors, or of pangenes; all of them essentially morphological conceptions; the products of imagination projected beyond the confines of the visible, yet always restrained by having only one source of experience—namely, the visible. One may give unstinted admiration to the brilliancy, and even set a high value on the usefulness, of these attempts to give formal representations of the genesis of organic structure, and yet recognise that their chief utility has been to make us realise more clearly the problems that have yet to be solved.

Stripped of all the verbiage that has accumulated about them, the simple questions that lie immediately before us are: What are the causes which produce changes in the forms of animals and plants? Are they purely internal, and, if so, are their laws discoverable? Or are they partly or wholly external, and, if so, how far can we find relations of cause and effect between ascertained chemical and physical phenomena and the structural responses of living beings?

As an attempt to answer the last of these questions, we have the recent researches of the experimental morphologists and embryologists directed towards the very aim that Hofmeister proposed. Originally founded by Roux, the school of experimental embryology has outgrown its infancy and has developed into a vigorous youth. It has produced some very remarkable results, which cannot fail to exercise a lasting influence on the course of zoological studies. We have learnt from it a number of positive facts, from which we may draw very important conclusions, subversive of some of the most cherished ideas of whilom morphologists. It has been proved by experiment that very small changes in the chemical and physical environment may and do produce specific form-changes in developing organisms, and in such experiments the consequence follows so regularly on the antecedent that we cannot doubt that we have true relations of cause and effect. It is not the least interesting outcome of these experiments that, as Loeb has remarked, it is as yet impossible to connect in a rational way the effects produced with the causes which produced them, and it is also impossible to define in a simple way the character of the change so produced. For example, there is no obvious connection between the minute quantity of sulphates present in sea-water and the number and position of the characteristic calcareous spicules in the larva of a Sea-urchin. Yet Herbst has shown that if the eggs of Sea-urchins are reared in sea-water deprived of the needful sulphates (normally .26 per cent. magnesium sulphate and .1 per cent. calcium sulphate), the number and relative positions of these spicules are altered, and, in addition, changes are produced in other organs, such as the gut and the ciliated bands. Again, there is no obvious connection between the presence of a small excess of magnesium chloride in sea-water and the development of the paired optic vesicles. Yet Stockard, by adding magnesium chloride to sea-water in the proportion of 6 grams of the former to 100 c.c. of the latter, has produced specific effects on the eyes of developing embryos of the Minnow (*Fundulus heteroclitus*): the optic vesicles, instead of

being formed as a widely separated pair, were caused to approach the median line, and in about fifty per cent. of the embryos experimented upon the changes were so profound as to give rise to cyclopean monsters. Many other instances might be cited of definite effects of physical and chemical agencies on particular organs, and we are now forced to admit that inherited tendencies may be completely overcome by a minimal change in the environment. The nature of the organism, therefore, is not all-important, since it yields readily to influences which at one time we should have thought inadequate to produce perceptible changes in it.

It is open to anyone to argue that, interesting as experiments of this kind may be, they throw no light on the origin of permanent—that is to say, inheritable—modifications of structure. It has for a long time been a matter of common knowledge that individual plants and animals react to their environment, but the modifications induced by these reactions are somatic; the germ-plasm is not affected, therefore the changes are not inherited, and no permanent effect is produced in the characters of the race or species. It is true that no evidence has yet been produced to show that form-changes as profound as those that I have mentioned are transmitted to the offspring. So far the experimenters have not been able to rear the modified organisms beyond the larval stages, and so there are no offspring to show whether cyclopean eyes or modified forms of spicules are inherited or not. Indeed, it is possible that the balance of organisation of animals thus modified has been upset to such an extent that they are incapable of growing into adults and reproducing their kind.

But evidence is beginning to accumulate which shows that external conditions may produce changes in the germ-cells as well as in the soma, and that such changes may be specific and of the same kind as similarly produced somatic changes. Further, there is evidence that such germinal changes are inherited—and, indeed, we should expect them to be, because they are germinal.

The evidence on this subject is as yet meagre, but it is of good quality and comes from more than one source.

There are the well-known experiments of Weismann, Standfuss, Merrifield, and E. Fischer on the modification of the colour patterns on the wings of various Lepidoptera.

In the more northern forms of the fire-butterfly, *Chrysophanus* (*Polyommatus*) *phleas*, the upper surfaces of the wings are of a bright red-gold or copper colour with a narrow black margin, but in Southern Europe the black tends to extend over the whole surface of the wing, and may nearly obliterate the red-gold colour. By exposing pupæ of caterpillars collected at Naples to a temperature of 10° C. Weismann obtained butterflies more golden than the Neapolitan, but blacker than the ordinary German race, and conversely, by exposing pupæ of the German variety to a temperature of about 38° C., butterflies were obtained blacker than the German, but not so black as the Neapolitan variety. Similar deviations from the normal standard have been obtained by like means in various species of *Vanessa* by Standfuss and Merrifield. Standfuss, working with the small tortoiseshell butterfly (*Vanessa urticae*), produced colour aberrations by sub-

jecting the pupæ to cold, and found that some specimens reared under normal conditions from the eggs produced by the aberrant forms exhibited the same aberrations, but in a lesser degree. Weismann obtained similar results with the same species. E. Fischer obtained parallel results with *Arctia caja*, a brightly coloured diurnal moth of the family *Bombycidae*. Pupæ of this moth were exposed to a temperature of 8° C., and some of the butterflies that emerged were very dark-coloured aberrant forms. A pair of these dark aberrants were mated, and the female produced eggs, and from these larvæ and pupæ were reared at a normal temperature. The progeny was for the most part normal, but some few individuals exhibited the dark colour of the parents, though in a less degree. The simple conclusions to be drawn from the results of these experiments is that a proportion of the germ-cells of the animals experimented upon were affected by the abnormal temperatures, and that the reaction of the germ-cells was of the same kind as the reaction of the somatic cells and produced similar results. As everybody knows, Weismann, while admitting that the germ-cells were affected, would not admit the simple explanation, but gave another complicated and, in my opinion, wholly unsupported explanation of the phenomena.

In any case this series of experiments was on too small a scale, and the separate experiments were not sufficiently carefully planned to exclude the possibility of error. But no objection of this kind can be urged against the careful and prolonged studies of Tower on the evolution of chrysomelid beetles of the genus *Leptinotarsa*. *Leptinotarsa*—better known, perhaps, by the name *Doryphora*—is the potato-beetle, which has spread from a centre in North Mexico southwards into the Isthmus of Panama and northwards over a great part of the United States. It is divisible into a large number of species, some of which are dominant and widely ranging; others are restricted to very small localities. The specific characters relied upon are chiefly referable to the coloration and colour patterns of the epinotum, pronotum, elytra, and under side of the abdominal segments. In some species the specific markings are very constant, in others, particularly in the common and wide-ranging *L. decemlineata*, they vary to an extreme degree. As the potato-beetle is easily reared and maintained in captivity, and produces two broods every year, it is a particularly favourable subject for experimental investigation. Tower's experiments have extended over a period of eleven years, and he has made a thorough study of the geographical distribution, dispersal, habits, and natural history of the genus. The whole work appears to have been carried out with the most scrupulous regard to scientific accuracy, and the author is unusually cautious in drawing conclusions and chary of offering hypothetical explanations of his results. I have been greatly impressed by the large scale on which the experiments have been conducted, by the methods used, by the care taken to verify every result obtained, and by the great theoretical importance of Tower's conclusions. I can do no more now than allude to some of the most remarkable of them.

After showing that there are good grounds for believing that
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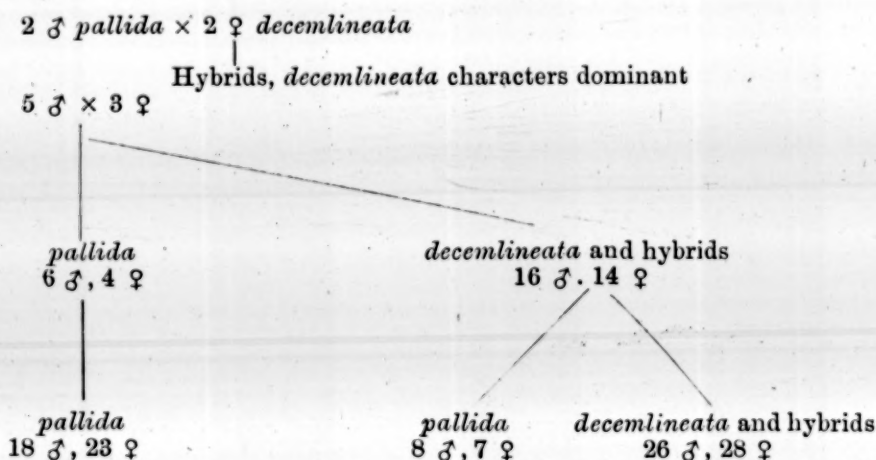
colour production in insects is dependent on the action of a group of closely related enzymes, of which chitase, the agent which produces hardening of chitin, is the most important, Tower demonstrates by a series of well-planned experiments that colours are directly modified by the action of external agencies—*viz.* temperature, humidity, food, altitude, and light. Food chiefly affects the subhypodermal colours of the larvæ, and does not enter much into account; the most important agents affecting the adult coloration being temperature and humidity. A *slight* increase or a *slight* decrease of temperature or humidity was found to stimulate the action of the colour-producing enzymes, giving a tendency to melanism; but a large increase or decrease of temperature or humidity was found to inhibit the action of the enzymes, producing a strong tendency to albinism.

A set of experiments was undertaken to test the question whether coloration changes induced by changed environmental conditions were inherited, increased, or dropped in successive generations. These experiments, carried on for ten lineal generations, showed that the changed conditions immediately produced their maximum effect; that they were purely somatic and were not inherited, the progeny of individuals which had been exposed to changed conditions through several generations promptly reverting when returned to normal conditions of environment. So far the results are confirmatory of the well-established proposition that induced somatic changes are not inheritable.

But it was found necessary to remove the individuals experimented upon from the influence of changed conditions during the periods of growth and maturation of the germ-cells. Potato-beetles emerge from the pupa or from hibernation with the germ-cells in an undeveloped condition, and the ova do not all undergo their development at once, but are matured in batches. The first batch matures during the first few days following emergence, then follows an interval of from four to ten days, after which the next batch of eggs is matured, and so on. This fact made it possible to test the effect of altered conditions on the maturing germ-cells by subjecting its imagoes to experimental conditions during the development of some of the batches of ova and to normal conditions during the development of other batches.

In one of the experiments four male and four female individuals of *L. decemlineata* were subjected to very hot and dry conditions, accompanied by low atmospheric pressure, during the development and fertilization of the first three batches of eggs. Such conditions had been found productive of albinic deviations in previous experiments. As soon as the eggs were laid they were removed to normal conditions, and the larvæ and pupæ reared from them were kept in normal conditions. Ninety-eight adult beetles were reared from these batches of eggs, of which eighty-two exhibited the characters of an albinic variety found in nature and described as a species under the name *pallida*; two exhibited the characters of another albinic species named *immaculothorax*, and fourteen were unmodified *decemlineata*. This gave a clear indication that the altered conditions had produced modifications in the germ-cells which were expressed by colour

changes in the adult individuals reared from them. To prove that the deviations were not inherent in the germ-plasm of the parents, the latter were kept under normal conditions during the periods of development and fertilization of the last two batches of eggs; the larvæ and pupæ reared from these eggs were similarly subjected to normal conditions, and gave rise to sixty-one unmodified *decemlineata*, which, when bred together, came true to type for three generations. The *decemlineata* forms produced under experimental conditions also came true to type when bred together. Of the *pallida* forms produced by experimental conditions all but two males were killed by a bacterial disease. These two were crossed with normal *decemlineata* females, and the result was a typical Mendelian segregation, as shown by the following table:—



This is a much more detailed experiment than those of Standfuss, Merrifield, and Fisher, and it shows that the changes produced by the action of altered conditions on the maturing germ-cells were definite and discontinuous, and therefore of the nature of mutations in De Vries' sense.

In another experiment Tower reared three generations of *decemlineata* to test the purity of his stock. He found that they showed no tendency to produce extreme variations under normal conditions. From this pure stock seven males and seven females were chosen and subjected during the maturation periods of the first two batches of ova to hot and dry conditions. Four hundred and nine eggs were laid, from which sixty-nine adults were reared, constituted as follows:—

Twenty (12 ♂, 8 ♀) . . .	apparently normal <i>decemlineata</i> .
Twenty-three (10 ♂, 13 ♀) . . .	<i>pallida</i> .
Five (2 ♂, 3 ♀) . . .	<i>immaculothorax</i> .
Sixteen (9 ♂, 7 ♀) . . .	<i>albida</i> .

These constituted lot A.

The same seven pairs of parents subjected during the second half of the reproductive period to normal conditions gave eight hundred and forty eggs, from which were reared one hundred and twenty-

three adults, all *decemlineata*. These constituted lot B. The *decemlineata* of lot A and lot B were reared side by side under normal and exactly similar conditions. The results were striking. From lot B normal progeny were reared up to the tenth generation, and, as usual in the genus, two generations were produced in each year. The *decemlineata* of lot A segregated into two lots in the second generation. A¹ were normal in all respects, but A², while retaining the normal appearance of *decemlineata*, went through five generations in a year, and this for three successive years, thus exhibiting a remarkable physiological modification, and one without parallel in nature, for no species of the genus *Leptinotarsa* are known which produce more than two generations in the year. This experiment is a sufficient refutation of Weismann's argument that the inheritance of induced modifications in *Vanessa urticae* is only apparent, the phenomena observed being due to the inheritance of two kinds of determinants—one from dark-coloured forms which are phyletically the oldest, and the other from more gaily coloured forms derived from the darker forms. There is no evidence whatever that there was ever a species or variety of potato-beetle that produced more than two, or at the most, and then as an exception, three broods in a year.

The modified albinic forms in this last experiment of Tower's were weakly; they were bred through two or three generations and came true to type, but then died out. No hybridization experiments were made with them, but in other similar experiments, which I have not time to mention in detail, modified forms produced by the action of changed conditions gave typical Mendelian characters when crossed with unmodified *decemlineata*, thus proving that the induced characters were constant and heritable according to the regular laws.

I have thought it worth while to relate these experiments at some length, because they seem to me to be very important, and because they do not appear to have attracted the attention in this country that they deserve.

They are confirmed to a very large extent by the experiments of Professor Klebs on plants, the results of which were published this summer in the Croonian Lecture on "Alterations of the Development and Forms of Plants as a Result of Environment." As I have only a short abstract of the Croonian Lecture to refer to, I cannot say much on this subject for fear of misrepresenting the author; but, as far as I can judge, his results are quite consistent with those of Tower. *Sempervivum funckii* and *S. acuminatum* were subjected to altered conditions of light and nutrition, with the result that striking variations, such as the transformation of sepals into petals, of petals into stamens, of stamens into petals and into carpels, were produced. Experiments were made on *Sempervivum acuminatum*, with the view of answering the question whether such alterations of flowers can be transmitted. The answer was in the affirmative. The seeds of flowers artificially altered and self-fertilized gave rise to twenty-one seedlings, among which four showed surprising deviations of floral structure. In two of these seedlings all the flowers were greatly altered, and presented some of the modifications of the mother plant,

especially the transformation of stamens into petals. These experiments are still in progress, and it would perhaps be premature to lay too much stress upon them if it were not for the fact that they are so completely confirmatory of the results obtained by similar methods in the animal kingdom.

I submit to you that evidence is forthcoming that external conditions may give rise to inheritable alterations of structure. Not, however, as was once supposed, by producing specific changes in the parental soma, which changes were reflected, so to speak, upon the germ-cells. The new evidence confirms the distinctions drawn by Weismann between somatic and germinal variations. It shows that the former are not inherited, while the latter are; but it indicates that the germ may be caused to vary by the action of external conditions in such a manner as to produce specific changes in the progeny resulting from it. It is no more possible at the present time to connect rationally the action of external conditions on the germ-cells with the specific results produced in the progeny than it is possible to connect cause with effect in the experiments of Herbst and Stockard; but, when we compare these two kinds of experiments, we are no longer able to argue that it is inconceivable that such and such conditions acting on the germ-plasm can produce such and such effects in the next generation of adults. We must accept the evidence that things which appeared inconceivable do in fact happen, and in accepting this we remove a great obstacle from the path of our inquiries, and gain a distinct step in our attempts to discover the laws which determine the production of organic form and structure.

But such experiments as those which I have mentioned only deal with one aspect of the problem. They tell us about external conditions and the effects that they are observed to produce upon the organism. They give us no definite information about the internal changes which, taken together, constitute the response of the organism to external stimuli. As Darwin wrote, there are two factors to be taken into account—the nature of the conditions and the nature of the organism—and the latter is much the more important of the two. More important because the reactions of animals and plants are manifold; but, on the whole, the changes in the conditions are few and small in amount. Morphology has not succeeded in giving us any positive knowledge of the nature of the organism, and in this matter we must turn for guidance to the physiologists, and ask of them how far recent researches have resulted in the discovery of factors competent to account for change of structure. Perhaps the first step in this inquiry is to ask whether there is any evidence of internal chemical changes analogous in their operation to the external physical and chemical changes which we have been dealing with.

There is a great deal of evidence, but it is extremely difficult to bring it to a focus and to show its relevancy to the particular problems that perplex the zoologist. Moreover, the evidence is of so many different kinds, and each kind is so technical and complex, that it would be absurd to attempt to deal with it at the end of an address that has already been drawn out to sufficient length. But

perhaps I may be allowed to allude to one or two generalisations which appear to me to be most suggestive.

We shall all agree that, at the bottom, production and change of form is due to increase or diminution of the activities of groups of cells, and we are aware that in the higher animals change of structure is not altogether a local affair, but carries with it certain consequences in the nature of correlated changes in other parts of the body. If we are to make any progress in the study of morphogeny, we ought to have as exact ideas as possible as to what we mean when we speak of the activities of cells and of correlation. On these subjects physiology supplies us with ideas much more exact than those derived from morphology.

It is, perhaps, too sweeping a generalisation to assert that the life of any given animal is the expression of the sum of the activities of the enzymes contained in it, but it seems well established that the activities of cells are, if not wholly, at all events largely, the result of the actions of the various kinds of enzymes held in combination by their living protoplasm. These enzymes are highly susceptible to the influence of physical and chemical media, and it is because of this susceptibility that the organism responds to changes in the environment, as is clearly illustrated in a particular case by Tower's experiments on the production of colour changes in potato-beetles. Bayliss and Starling have shown that in lower animals, protozoa and sponges, in which no nervous system has been developed, the response of the organism to the environment is effected by purely chemical means. In protozoa, because of their small size, the question of coadaptation of function hardly comes into question; but in sponges, many of which are of large size, the mechanism of coadaptation must also be almost exclusively chemical. Thus we learn that the simplest and, by inference, the phyletically oldest mechanism of reaction and co-ordination is a chemical mechanism. In higher animals the necessity for rapid reaction to external and internal stimuli has led to the development of a central and peripheral nervous system, and as we ascend the scale of organisation, this assumes a greater and greater importance as a co-ordinating bond between the various organs and tissues of the body. But the more primitive chemical bond persists, and is scarcely diminished in importance, but only overshadowed by the more easily recognisable reactions due to the working of the nervous system. In higher animals we may recognise special chemical means whereby chemical coadaptations are established and maintained at a normal level, or under certain circumstances altered. These are the internal secretions produced by sundry organs, whether by typical secretory glands (in which case the internal secretion is something additional and different from the external secretion), or by the so-called ductless glands, such as the thyroid, the thymus, the adrenal bodies, or by organs which cannot strictly be called glands—namely, the ovaries and testes. All these produce chemical substances which, passing into the blood or lymph, are distributed through the system, and have the peculiar property of regulating or exciting the specific functions of other organs. Not, however, of all the organs, for the

different internal secretions are more or less limited and local in their effects: one affecting the activity of this and another the activity of that kind of tissue or organ. Starling proposed the name hormones for the internal secretions, because of their excitatory properties (*ὀρμῶν*, to stir up, to excite).

Hormones have been studied chiefly from the point of view of their stimulating effect on the metabolism of various organs. From the morphologist's point of view, interest chiefly attaches to the possibility of their regulating and promoting the production of form. It might be expected that they should be efficient agents in regulating form, for, if changes in structure are the result of the activities of groups of cells, and the activities of cells are the results of the activities of the enzymes which they contain, and if the activities of the enzymes are regulated by the hormones, it follows that the last-named must be the ultimate agents in the production of form. It is difficult to obtain distinct evidence of this agency, but in some cases at least the evidence is sufficiently clear. I will confine myself to the effects of the hormones produced by the testes and ovaries. These have been proved to be intimately connected with the development of secondary sexual characters—such, for instance, as the characteristic shape and size of the horns of the bull; the comb, wattles, spurs, plumage colour, and spurs in poultry; the swelling on the index finger of the male frog; the shape and size of the abdominal segments of crabs. These are essentially morphological characters, the results of increased local activity of cell-growth and differentiation. As they are attributable to the stimulating effect of the hormone produced by the male organ in each species, they afford at least one good instance of the production of a specific change of form as the result of an internal chemical stimulus. We get here a hint as to the nature of the chemical mechanism which excites and correlates form and function in higher organisms; and, from what has just been said, we perceive that this is the most primitive of all the animal mechanisms. I submit that this is a step towards forming a clear and concrete idea of the inner nature of the organism. There is one point, and that a very important one, upon which we are by no means clear. We do not know how far the hormones themselves are liable to change, whether by the action of external conditions or by the reciprocal action of the activities of the organs to which they are related. It is at least conceivable that agencies which produce chemical disturbances in the circulating fluids may alter the chemical constitution of the hormones, and thus produce far-reaching effects. The pathology of the thyroid gland gives some ground for belief that such changes may be produced by the action of external conditions. But, however this may be, the line of reasoning that we have followed raises the expectation that a chemical bond must exist between the functionally active organs of the body and the germ-cells. For if, in the absence of a specialised nervous system, the only possible regulating and coadapting mechanism is a chemical mechanism, and if the specific activities of a cell are dependent on the enzymes which it holds in combination, the germ-cells of any given animal must be the depository of a stock of

enzymes sufficient to insure the due succession of all its developmental stages as well as of its adult structure and functions. And as the number of blastomeres increases, and the need for co-ordination of form and function arises, before ever the rudiments of a nervous system are differentiated, it is necessary to assume that there is also a stock of appropriate hormones to supply the chemical nexus between the different parts of the embryo. The only alternative is to suppose that they are synthesised as required in the course of development. There are grave objections to this supposition. All the evidence at our disposal goes to show that the potentialities of germ-cells are determined at the close of the maturation divisions. Following the physiological line of argument, it must be allowed that in this connection "potentiality" can mean nothing else than chemical constitution. If we admit this, we admit the validity of the theory advanced by more than one physiologist, that heritable "characters" or "tendencies" must be identified with the enzymes carried in the germ-cells. If this be a true representation of the facts, and if the most fundamental and primitive bond between one part of an organism and another is a chemical bond, it can hardly be the case that germ-cells—which, *inter alia*, are the most primitive, in the sense of being the least differentiated, cells in the body—should be the only cells which are exempt from the chemical influences which go to make up the co-ordinate life of the organism. It would seem, therefore, that there is some theoretical justification for the inheritance of induced modifications, provided that these are of such a kind as to react chemically on the enzymes contained in the germ-cells.

One further idea that suggests itself to me and I have done. Is it possible that different kinds of enzymes exercise an inhibiting influence on one another; that germ-cells are "undifferentiated" because they contain a large number of enzymes, none of which can show their activities in the presence of others, and that what we call "differentiation" consists in the segregation of the different kinds into separate cells, or perhaps, prior to cell-formation, into different parts of the fertilised ovum, giving rise to the phenomenon known to us as prelocalisation? The idea is purely speculative; but, if it could be shown to have any warrant, it would go far to assist us in getting an understanding of the laws of the production of form.

I have been wandering in territories outside my own province, and I shall certainly be told that I have lost my way. But my thesis has been that morphology, if it is to make useful progress, must come out of its reserves and explore new ground. To explore is to tread unknown paths, and one is likely to lose one's way in the unknown. To stay at home in the environment of familiar ideas is no doubt a safe course, but it does not make for advancement. Morphology, I believe, has as great a future before it as it has a past behind it, but it can only realise that future by leaving its old home, with all its comfortable furniture of well-worn rules and methods, and embarking on a journey, the first stages of which will certainly be uncomfortable and the end is far to seek.